MEMORANDUM
RM-3425
MAY 1963

NOTES OF WORKING SYMPOSIUM ON SOLAR SYSTEM CONSTANTS, FEBRUARY 22-26, 1962

Compiled by Donna Wilson

The RAND Corporation
SANTA MONICA • CALIFORNIA

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PREFACE

In connection with a long-range research study involving analyses of the constants and parameters of the solar system, the symposium described in this Memorandum was held at RAND in February 1962. The purpose of the symposium was to clarify problem areas underlying an attempt to devise a consistent set of fundamental astronomical constants, taking into account present and future needs of space flight. Throughout the discussions, the theme was primarily to formulate problems and approaches to the solution of these problems rather than to report on the latest values and results of research. It should be stressed that this was a working, rather than a reporting, symposium. The participants did not present papers, but rather engaged in a series of discussions in plenary sessions and separate working groups under the headings shown on p. vii.

In order to promote free discussion, the sessions were kept informal, and no formal record of the symposium was made at the time. The notes presented here have been prepared from personal notes of the RAND staff members who attended, for the benefit of those working on solar system constants. A draft copy was circulated to all participants, and their respective corrections have been made in this Memorandum. The unanimous opinion expressed on whether or not these notes should be distributed to others was that they were valuable to persons who attended, but for nonparticipants they were an uneconomical source for extracting particular information. Even so, no one objected to making the notes available to other interested researchers. Although the various RAND staff members (especially R. T. Gabler, J. W. Kern, G. F. Schilling, F. T. Smith, D. W. Stebbins, and A. G. Wilson) have contributed immeasurably to the continuity and accuracy of the Memorandum, the compiler assumes full responsibility for the final form.

These notes will mainly be of use to the symposium participants in their individual researches; however, they will also be made available to others who can benefit from the discussions. The compiler would emphasize that the intent underlying the preparation of this Memorandum has been to preserve the unanswered questions rather than to produce a finished product.

The symposium was supported in part by the National Aeronautics and Space Administration under Contract NASr-21(04); in part by the Jet Propulsion Laboratory, California Institute of Technology, under Contract N-33561 (NAS7-100); and in part by U.S. Air Force Project RAND.

INTRODUCTORY NOTE

As a result of the informal discussions and deliberations by the symposium participants, who represented the various disciplines of astronomy, geodesy, geophysics, and space flight exploration, it was generally recognized that there exist two distinct and quite different requirements on systems or sets of astronomical constants. For example, the necessity for consistency in the values of all theoretically related constants does not fill the same role for space exploration as for ephemerides. A self-consistent set leading to the best approximations of positions of celestial objects over long periods of time is one need. The most accurate position for a specified time based on the best available data is a separate, but very real, need in particular cases of spacecraft design.

Many problems were considered in detail, and the following major items were discussed:

1. Fixing a geocentric gravitational constant similar to the Gaussian constant with a unit of distance defined for geocentric orbits would be advantageous to orbit computations of earth satellites. This unit should be considered as approximating but distinct from the physical size of the earth.

2. The need for adopting a consistent set of constants which may be used for computational purposes to facilitate direct comparison of results was stressed even though this set may not always satisfy the requirements of individual users in specific cases. The need for and difficulties in updating the system to incorporate new observations was felt to be one of the central problems requiring early solution. In this connection it was also felt that the importance of having authors publish the numerical values of all astronomical and physical constants used in their reductions should be emphasized.

3. New techniques such as radar range and doppler data used for distance determinations in the solar system introduce the possibility of improving the older methods of orbit determinations through simultaneous optical and radar observations. Before a complete knowledge of the limitations of these methods singly or in combination can be

realized, it will be necessary to have both optical and radar observations of various objects over the greatest arcs possible.

4. Although space missions may primarily serve other purposes, additional observational data necessary for improving knowledge of astronomical and physical constants in many cases could be obtained from them. Missions should be reviewed for this possibility.

It is felt that the symposium fulfilled its primary purpose of clarifying many of the underlying questions and delineating the problem areas in which research effort should yield results helpful in meeting the needs of space flight design and in contributing to basic understanding of the solar system.

Included in this Memorandum are editorial notes and definitions, a subject index, a name index, and a bibliography of 40 entries pertaining to solar system constants and related subjects.

CONTENTS

PARTICIPANTS

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Gerald M. Clemence, Naval Observatory
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Patricia A. Walters, The RAND Corporation
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Invited Guests

Neil Block, Jet Propulsion Laboratory
Rowland Carpenter, Jet Propulsion Laboratory
Victor C. Clarke, Jet Propulsion Laboratory
E. Freeman, Jet Propulsion Laboratory
Douglas Holdridge, Jet Propulsion Laboratory
L. Wong, Space Technology Laboratories

PLENARY SESSION
Thursday Morning, 2-22-62
(A. Wilson, Chairman)

Welcoming Address - George H. Clement (Appendix A)

Introductory Address - Albert G. Wilson (Appendix B)

I. PHILOSOPHIES OF APPROACH; SYSTEMS OF CONSTANTS

A. Wilson In anticipating space age requirements for fundamental astronomical constants, several factors affect our considerations: the rate of development of space technology is much faster than anticipated; explosive developments are taking place in the field of computers; and new observational devices such as radar and masers afford new methods of determining constants.[1]*

The "constants problem" divides itself into three basic points of view. The point of view of:

1. The users (applied astro-dynamics) who are interested in "best" values for specific missions.
2. Solar system scientists who are concerned with the area of basic science (physics, geophysics, etc. of satellites, planets, comets, etc.).
3. The custodians of ephemerides and constants who are required to prepare and maintain ephemerides and synthesize observations and theory into a consistent whole.

From the point of view of the techniques, there exist such problems as the discrepancies between the radar determinations of the a.u. and the optical determinations. There is also the question of whether and how often new values of constants should be adopted.

Question Several constants have numerical values which are known to a great many significant figures in units of the a.u. and solar mass, for example. What about the requirement of determining the values of these constants in laboratory units?

Herrick We need to know more about those laboratory units. It is not a question of disparaging either system; it is a question of obtaining the ratio of the a.u. to cm. The term "best" value is a matter of

*Numbers in brackets refer to editorial notes in Appendix C.

usage. Astronomers have been more concerned with long term consistency. Over a long period of years observations can be related to ephemerides, but this is not sufficient for space navigation. We need a consistent set of constants for ephemeris purposes, but we also need a set of "best" values for space navigation purposes.[2] In a discussion several years ago, Professor Leuschner emphasized the need for ephemerides over a long period of time (say 50 years) with regard to the theory of general perturbations of minor planets. He criticized changing constants at each epoch for fear of losing certain minor planets. However, today we must also attempt to provide for the needs of the space age.

Another factor is the relationships between constants. These are of two kinds: relationships of scientific interest per se and those which are of interest only in so far as they affect the determination of constants from observations. The relationship of the equatorial radius and the harmonic terms is of practical importance and not necessarily independent. We should agree not only on current "best" values but also on which relationships are important. For example, we should examine the expressions of the earth's equatorial radius and the gravity potential to determine if it is necessary to make use of certain observationally determined quantities, such as the earth's mean radius, in reducing this relationship to the simplest terms.

With regard to the solar parallax, there are two values which give the ratio of the a.u. to the cm: (1) radar measurements and (2) optical determinations. We must find the explanation for the systematic differences between the two. We now have the question of the ratio of the light-sec to the a.u., the question of the light-sec to the kilometer, and the question of the kilometer to the a.u. There is a need for clarifying these relationships.

<u>Brouwer</u> The problem of today distinguished from the problem prior to the space age is that before it was entirely passive. Until twenty years ago, a consistent set of constants was completely satisfactory for ephemeris purposes, except for a few theoretical inconsistencies and disagreement between the observed and computed

value of the constant of nutation. (We cannot consider the motion of the earth as a rigid body.) Astronomers were well-satisfied if the theory was satisfied. This is a conservative approach. Today with space needs we may need two sets of constants. Current space activities may require changing values of constants while ordinary ephemerides require a consistent set with no need of piecemeal adjustment.

Gates Speaking as user, the problem of how constants affect space flight requires that we ask what measurements are made. In a typical flight to Mars, for example, we presently make measurements on the spacecraft from the earth, via radar; hence earth GM and the ephemeris of the earth are important, as well as the ephemeris of Mars. In the future, however, we may make measurements from within the spacecraft, in which case as we approach Mars its mass and radius may control our accuracy.

Baker We should not limit the kind of data we will utilize. Future systems will measure range and range-rate data, angular data such as planets against a star background, and even 21-cm line data. We do not now know all the possibilities. As new systems evolve we should be able to use all kinds of data.

Question Maybe the ratio between the a.u. and laboratory units is not needed?

Herrick Today the ratio of the a.u. to the light-sec is needed. Historically the solar parallax was adopted because early determinations involved the radius of earth. Today the a.u. is more important than the solar parallax.[3]

de Vaucouleurs With regard to the solar parallax, there is only one method, the classical trigonometric parallax, which gives the solar distance through a relation involving the equatorial radius of the earth. Other methods such as the dynamical method, which determines the ratio of the mass of the sun to the mass of the earth, do not involve the angular value of the earth's equatorial radius (although it is customary to express the results as a "parallax" value). All "physical" methods give the a.u. in units of light-time.

Discussion A joint discussion began on which methods measured what, e.g., the parallactic inequality gives angular measurement. As long as we have formulas which give relations it does not matter what one measures. For instance, Rabe's method gives the ratio of the a.u. to the earth's equatorial radius, radar methods give the ratio of the a.u. to the "light second" as we measure it in space. We no longer need be concerned with an angular value of the solar parallax.

Question How accurately do we need to know the distance between planets in kilometers?

Gates There is no answer to this question. Any uncertainty, if properly described, can be circumvented, although at the expense of increased complexity. In a ballistic flight to Venus the launch errors may introduce a miss distance of 250,000 km or more. Through radar tracking and midcourse guidance, the miss distance can be reduced to 5000 km or better. On the other hand, errors due to uncertainties in the astronomical constants may lead to errors on a similar flight of only 2500 km depending on whose a.u. we adopt. We should like to reduce the miss distance. We plan to achieve the required accuracy by measuring angles such as the angle between the destination planet and a star or the sun. We overcome the need for accurate distances between planets, therefore, by terminal guidance.

Question Will the experience gained in completing such flights enable us to increase the accuracy of the known distances?

Clemence I am very pessimistic about determining astronomical constants from space probes because of contradictory results. A probe supplies only a small portion of arc, which in general does not yield enough information. Rather, we need to have vehicles orbiting the bodies in question in order to obtain repeated measurements.

Discussion Joint discussion followed on other perturbing effects on the probes, such as those from radiation. These effects are smaller than our present ability to measure them; however, the uncertainty in radiation pressure is four times greater than the uncertainty in the a.u.

Question What about the uncertainties due to the various definitions of time? Even though Atomic Time is given to eight or nine significant figures, with the prospect of 10 places soon, we do not know

Ephemeris Time at any given instant.[4]

Clemence There are limitations on the accuracy of direct determination of Ephemeris Time. We do, however, know the relationship between Ephemeris Time and Universal Time and also the relations between Universal Time and Atomic Time. We can therefore be as precise as we wish, since the differences can be made as small as the relative accuracies. That is to say, we know any deviation in the earth's motion in terms of Atomic Time to nine or ten significant figures. UT has no place in space travel. We will use ET, which now can be related to the time kept by atomic clocks.

Coffee Break

Clemence A review of de Sitter's paper*

The set of constants used in this paper can be defined as a set because: (1) There are theoretical interrelations which exist among them, and (2) they constitute a model of the earth and its motions.

The first systematic treatment of this subject was given by William Harkness.** The paper by Newcomb (published in the 1897 Supplement of the American Ephemeris) entitled "The Elements of the Four Inner Planets and Fundamental Constants of Astronomy," is another illuminating treatment. De Sitter's paper provides two landmarks: the comprehensive treatment, which considers the departures of the earth's motion from the motion of a rigid body, and the logical method, which chooses eight fundamental constants and derives, in terms of these, values of other constants. De Sitter introduces symbolic corrections to all of these values, and the expressions given (on pp. 230-231) make it possible to see the effect of changing the values of these constants.

It should be noted that any list of fundamental constants is somewhat arbitrary. The choice is not unique. The masses of planets are necessary for calculating planetary precession. The obliquity

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*"On the System of Astronomical Constants," Bull. Astron. Inst. Netherlands, Vol. 8, No. 307, July 8, 1938, pp. 213-231.

**"The Solar Parallax and its Related Constants, Including the Figure and Density of the Earth," Washington Observations for 1885, Appendix III, 1891.

of the ecliptic and the eccentricity of the earth's orbit might also have been included, although they were not.

There are three distinct values of constants to be distinguished:

1. <u>conventional or adopted</u> -- these values are a matter of international agreement and form the basis for the various ephemerides.
2. <u>observed</u> -- these values change with new determinations.
3. <u>adjusted</u> -- these are values that rigorously satisfy theoretical values to agree within observational errors. As a general rule the observational errors are greater than can be explained.

The system of constants used in de Sitter's paper (p. 230) <u>does</u> satisfy de Sitter's theory rigorously. The constants (K and λ) are two small constants related to the internal structure of the earth. It is not necessary to develop the theory in this form. It could have been developed equally as well using the gravitational constant of the earth in spherical harmonics (with one exception, i.e., in the case of events such as eclipses that occur on the earth).

In passing, a word about the mean motions: the figure given for planetary precession is incorrect in the last 2-3 figures, and the coefficient of T in the obliquity of the ecliptic is incorrect in the last 3 figures. These errors affect the remainder of the work. (They are due to Newcomb's theory of the motion of the earth about the sun, which is first-order theory.) However, we have better figures coming in approximately two years. The coefficient of T in the obliquity of the ecliptic requires correction. In all three cases the coefficients in T^2 need to be substantially altered.

Next spring (1963) an IAU symposium on constants will consider the adoption of a new conventional system. The present system is defective in several respects:

1. Some values are wrong: a_e, g_e, and c.

2. The most troublesome value is the constant of precession. This value has more far-reaching consequences since all star catalogs (since 1900) depend on it. All observations of star positions,

as well as the position of planets that have been reduced to apparent place, are brought together by use of the constant of precession. The introduction of a correction to this constant will introduce a discontinuity in angular astronomical observations, and then there is the question of the extent of rework which will be requuried.

Question Isn't it possible to apply backward corrections?

Clemence Yes, but it could conceivably make more work in the end, and perhaps we should not introduce a change until 1999.

Question Are there not other instances where corrections have introduced discontinuities, such as the change in the moon's position made some two years ago in the ephemerides?

Clemence Yes, although only parts of the ephemeris were affected. The ephemeris of the moon and the elements of the outer planets Jupiter, Saturn, Uranus, Neptune, and Pluto are the only parts that have been changed since 1960. The other planets are the same.

Herrick It is important to draw a line of demarcation between the function of the IAU in making "magnificent readjustments" versus the practical needs of navigation. On the question of the astronomical reference system--many engineers coming into these problems from the rocket business several years ago did not appreciate the astronomical system (i.e., the mean equator of 1950). It does not seem that any outside group can supply an input to this system--this should be left to the IAU. However, the other questions of practical navigation should bring in the users to determine "best" current values. Perhaps we should establish a working group to handle this problem.

In reference to the de Sitter/Brouwer paper, the concept of correction terms is extremely important. We need to reconsider these expressions; for instance, K is only one way of expressing the departure of the earth from the ellipsoid. It is determined theoretically and subject to observational determination from the 4th harmonic. If we introduce 3rd or 6th harmonics, we need additional K's or coefficients. It is a geodetic problem. We also need to do some work on a_e, the earth's equatorial radius. De Sitter says that the mean radius is better determined than the equatorial radius. This is true because observations have been made to determine the mean radius. As a connective between certain constants however, it does not provide better accuracy. The mean radius only complicates these relationships. I want to be open minded on this subject, but should we keep the mean radius?

Question Would you suggest a second set of constants--an updated set? Or would you suggest a "best" set for a given purpose?

Herrick We may have to modify relationships--perhaps a "best" set for a given purpose.

Clemence This would mean a different set for every user.

Herrick But we are all interested, for example, in where Venus really is. Let's figure out what sets are needed.

de Vaucouleurs A "best" value can be defined as:

1. That value chosen after all the evidence has been brought together, or
2. That value which is part of a consistent set, i.e., an adjusted value.

Brouwer Suppose the IAU committee comes up with recommendations? These may evolve into a satisfactory set by 1964, but it will only cause new problems for the users.

Baker I'm concerned with a series of ad hoc constant determinations for every given user. Are there a limited number of users? Let all the users work with the same data just for the sake of consistency and comparability of results. There is always a trade-off between values and observations.

de Vaucouleurs There is another general point which should be made. It is difficult for one not working every day with constants to see all the implications of a change in the value of one. While much can be said for changing one, the same is true for leaving it alone if the others are not consistently adjusted as well.

A. Wilson Perhaps a process can be derived. While values cannot be changed at random, it is important to keep them updated.

At this time, we must interrupt the discussion and turn to the agenda; I would like to call on all the participants for their inputs to the agenda.

Specific requests:

1. Discussion on planetary constants.
2. Discussion of the value of probes in the determination of constants. Also discussion of choice of orbits--it may be that some orbits are better than others for constant determination.
3. Discussion of the value of continued radar observations and the relation of these to optical observations.
4. Discussion of the reason for using the light-sec as against using km.
5. Discussion of planetary masses and their determination.
6. Discussion of the precessional constant and determination of cometary orbits. The consideration of sending a space probe to intersect a cometary orbit.
7. A presentation of the JPL Venus radar determination of the a.u.
8. Discussion of the discrepancy in the mass of the Moon.
9. Discussion of recommended values and standardized lists for the user.
10. Discussion of the pole, the ephemerides, and the ellipticity of Mars, and the rotation of Venus.
11. Discussion of the expression of the earth's equatorial radius.
12. Discussion of the problem of handling experimental data statistically and the application of least squares.
13. Discussion of the dynamic figures of the earth.

II. PROBLEM AREAS RELATIVE TO THE ASTRONOMICAL UNIT

Clemence In beginning our discussion of the radar experiment to determine the a.u., I would like to ask what it is we need to know about the distance to Venus?

Gates In the JPL determination of the a.u., seventy degrees of the heliocentric arc of Venus was tracked. Doppler (Range Rate) accuracy was one part in 10^5. Verification of the signal from Venus was obtained by searching areas of the sky immediately surrounding Venus for spurious signals. Range was obtained by use of a pulse train. The experiment used an 85-ft parabolic antenna giving a beam width less than one degree at the operating frequency of 2388 mc.

Peabody The report, JPL Technical Report No. 32-221, "The Astronomical Unit Determined by Radar Reflections from Venus," D. B. Muhleman, D. R. Holdridge, and N. Block, supersedes JPL report TR 32-132, although it is not a final report, since it is based on Newcomb's ephemerides using Duncombe's corrections.* The question of all other radar determinations (i.e., those of MIT) and their probable error is not resolved. On the question of Duncombe's corrections, these are empirical corrections based on observations from 1750-1949 using E.T., and according to Brouwer, it is proper to apply these corrections.

Question How do you take into account the problem of knowing the position of Venus over a long period of time (which these corrections help to do), in contrast to the short range need of knowing the most accurate position for a small portion of the arc?

Brouwer A better method would be to obtain a set of equations to relate radar measurements to the optical data. The optical observations that have been used in Duncombe's discussion extend over a long period and are useful for determining the orbital elements and their secular perturbations.

*Duncombe, Raynor L., "The Motion of Venus, 1750-1949," Astronomical Papers, Vol. 16, 1959.

<u>Clemence</u> Newcomb's theories* are first order theories. The errors are appreciable in the case of Earth and of Mars. We know that the general effect of neglecting higher order terms is that errors increase with time. It might be useful to distinguish the error since 1900 and compare it with the error due to the whole range of observations (1750-1949) in order to determine weights.

<u>Peabody</u> The JPL observations extend from March 10 - May 10, 1961; conjunction was April 11. The range and doppler data are separate. Doppler data for relative velocity determinations were taken for a month on either side of conjunction. For some unknown reason the data taken prior to conjunction showed better consistency than the data taken after conjunction. During any one determination the frequency was constant to one part in 10^{10} during the time of signal travel to Venus and return. The radius of Venus used was 6100 km, and velocity of light used was 299,793.0 ± 0.3 km/sec. From the doppler data (before and after conjunction) the standard deviation of residuals was .14 km/sec. For the range data (after conjunction only) the standard deviation of residuals was 40 km. A numerical integration was fitted to the Newcomb-Duncombe ephemerides. The study used a partial set of corrections that were communicated personally by Duncombe. In the fall of 1962, new experiments involving radar reflection from both Mars and Venus are planned.

<u>Question</u> Would it be possible to make use of the JPL range and range-rate data to establish new information about the fine structure of both Earth's and Venus' ephemerides?

<u>Clemence</u> With regard to the fine structure--45 days is not enough. There are a few hundred periodic terms whose period is longer and hence cannot be determined in this length of time.

<u>Peabody</u> The following data of the determination of the a.u. is from the JPL Report TR 32-221. The probable error is statistical in nature as it is based on residuals of the data itself.

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*Newcomb, Simon, "Tables of the Four Inner Planets," <u>Astronomical Papers</u>, Vol. 6, 1895.

		Newcomb	Duncombe
I.	Doppler frequency near eastern elongation‡	149,597,550 ± 200	149,598,950 ± 200
II.	Doppler frequency near western elongation	149,599,650 ± 500	149,598,250 ± 500
III.	Open-loop range at conjunction	149,598,970 ± 100	149,598,930 ± 100
IV.	Closed-loop range at conjunction‡	149,599,150 ± 100	149,598,850 ± 100
V.	Long-count doppler near western elongation	149,599,750 ± 500	149,598,750 ± 500

‡I and IV are considered the best determinations

Arithmetic mean	149,598,754 ± 340
Weighted mean using reciprocal standard deviations	149,598,845 ± 180
Weighted mean using reciprocal variances	149,598,884 ± 126

Adopted values:

a.u. = 149,598,845 ± 250 km (p.e.)

Solar Parallax = 8".7940976 ± 147 (p.e.)

using a_e = 6,378,145 m and c = 299,793.0 = 0.3 km/sec

The ± 180 p.e. quoted above was increased to ± 250 because of the possible systematic errors.

Possible sources of error in the experiment, discussed in the report, are:

1. uncertainties in the theory of the planetary motion
2. systematic errors in extracting numerical quantities from the ephemerides
3. the uncertainty in the vacuum speed of light
4. dispersion effects on the speed of light
5. equipment biases and frequency drift

Herrick In Westrum's work on the MIT determination,* the sources of error considered were:

1. velocity of light
2. atmospheric effects
3. the radius of Venus

*Herrick, S., G. Westrum, and Maud Makemson, "The Astronomical Unit and the Solar Parallax," UCLA Astrodynamical Report No. 5, September 1959, pp. 11-15.

Question What is the effect of using a radius of 6100 km ± 50 km?

Peabody An error in the radius of 100 km would cause an error in the a.u. of 350 km at conjunction. Quoting from the JPL report, p. 17, "Some variations in the radius were studied, and it was found that the best agreement betwee the doppler and range determinations of the a.u. was obtained with a radius of 6100 ± 50 km. Smaller increments than 50 km were not studied because of the difficulty in comparing results."

Gates A point to be mentioned is that this method provides a means of determining the radius of Venus, since doppler measures to the center of mass while the range measures to the surface.

Question What about the fact that the JPL experiment was made at 2388 mc while MIT's was made with the 400 mc range?

Peabody Although there must be a frequency dependency in the experiment, it is not possible to say more until the experiment is repeated.

Question What about the utilization of radar measurements in the future?

Clemence Astronomers have for hundreds of years considered fresh observations to be useful. We still observe the planets at the Naval Observatory on all clear nights. Similarly, the radar methods will be worth the effort of repeating many times. An intensive program is called for.

For Mercury, from a standpoint of general relativity, there exists the well known secular perturbation of perihelion. In addition, there exists a perturbation of the radius vector, which is equal to 11.8 km (e cos M) and a perturbation in longitude equal to 23.7 km (e^2 cos M). It is not clear in the theory how distance is measured.[5] H. Jeffreys in "Scientific Inference"* discusses various meanings of distance in the relativistic equations.

Question What is the effect of relativity on range rate terms?

Clemence The optical and radar range determination of e of the orbit would yield different results.

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*Jeffreys, Sir H., Scientific Inference, Cambridge University Press, 1957.

<u>Question</u> What about radar methods for the planet Mars?

<u>Peabody</u> The problem with Mars is the acquisition ephemerides for lock on. It is not so much a problem of angle for this technique.

<u>BREAK</u>

(C. Gates, Chairman)

The manner in which physical constants affect spacecraft flight may be made clearer, perhaps, by considering the procedures for trajectory calculation, orbit determination, and spacecraft guidance currently in use:

1. First, the class of possible spacecraft trajectories is studied by computing approximate (conic) trajectories, using, however, the time ephemerides of the earth and the destination planet, which ephemerides are stored in the computer on tape. Such approximate trajectories from Earth to planet exist for the inner planets and for Jupiter and Saturn for about the next 15 years.

<u>Question</u> Are these trajectories restricted to favorable windows?

<u>Gates</u> In part. We do not compute trajectories for every conceivable launch date and flight time. However, the regions adjacent to the most favorable times are explored.

2. Next, trajectories suitable for a given spacecraft mission are selected, and these trajectories are computed as precisely as possible using integrated (Cowell or Encke) programs.
3. Following launch the spacecraft is tracked via radar. Range rate (doppler), angles (local hour-angle and declination) and, sometimes, range are obtained. The accuracy of doppler is very good, approaching 0.001 m/s (since uncertainty in the velocity of propagation limits this accuracy, a more proper unit would be light sec/sec). Angular measurements are not precise, errors being on the order of .1° to .2°, and are useful chiefly during the very early portions of flight. Range accuracy depends somewhat on the mechanization; 50-100 m has been discussed.

Direct optical tracking of the spacecraft has not been used to date, although the data would be of great importance in helping to determine the orbit. There are two reasons why optical tracking has not been employed, (a) the orbit determination accuracy available from radar observations appears adequate for the missions currently underway, and (b) by the time, in flight, that the spacecraft angular rate has diminished to a point where observation becomes convenient, the visual magnitude is slipping down into the noise--hence mirrors, lights, balloons, sodium clouds, etc. would be needed on the spacecraft.

4. The orbit is determined from the radar data by a conventional least squares fit. In addition to solving for the orbit, we also plan to fit on those (uncertain) physical constants which most strongly affect the trajectory and/or observations, including a.u., tracking station locations, GM of the earth, and velocity of propagation. Another important factor, which must be solved for and which is difficult to distinguish from an error in its effects, is the effective cross section of the spacecraft to solar radiation pressure.
5. Control of the attitude of the spacecraft is with respect to the Sun and a second body (usually Canopus, but sometimes Earth). Maneuvers to correct errors in the trajectory due to imperfect injection are computed on Earth and transmitted to the spacecraft via the radio link; a typical maneuver will be in the range of 10-30 m/sec. Miss distance errors will be due to geodetic errors, errors in the locations of the tracking stations, a.u. errors, Earth and planetary ephemeris errors, radar errors, and errors in the execution of the maneuvers. Typical accuracies (without terminal guidance) will be 20 km for the moon and 1000 km for Mars or Venus.

<u>Question</u> Navigation is in what units?

Gates The maneuver is in laboratory units; however, since the maneuver is a trim or vernier, this is not important.

Question In your opinion can we use the data to obtain a better value of the a.u.?

Gates Yes, since we are planning to fit on the a.u.

Question How does this compare with the dynamical method of STL?

Gates The method is similar, although we hope to track the spacecraft further--hopefully well beyond encounter with the planet. STL's determination was somewhat sensitive to the weights used, and we hope to be less sensitive in this respect.

Discussion A group discussion followed on the reliability of optical methods to determine the a.u. De Vaucouleurs briefly reviewed his paper, "The Astronomical Unit of Distance."*

Brouwer Every determination except Rabe's is unreliable!

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*Vaucouleurs, G. de, The Astronomical Unit of Distance, The RAND Corporation, RM-2944-NASA, December 1961.

PLENARY SESSION
Friday Morning, 2-23-62
(S. Herrick, Chairman)

III. PROBLEM AREAS OF GEODETIC AND GEOPHYSICAL CONSTANTS

Herrick The current values of geodetic constants and the interrelations between them are of special interest. I should like to discuss the work from the paper, "Gravitational and Related Constants for Accurate Space Navigation."*[6]

The earth's equatorial radius and the connection with the harmonics in the potential of the gravitational field are:

$$a_e = 6378\ 270\ (1 + a' + 4/3\ f')\ \text{meters}$$

where a' is a correction based on new observations and f' is the flattening correction. At that time (1957) the values were: for the flattening,

$$f = 1/297 = + 0.003,\ 367,000 + f';\ a' = 0 \pm 10 \times 10^{-6} \text{ and } f' = 0 \pm 4 \times 10^{-6}$$

$$J = 0.001,638,08 + f'$$

$$K = 9.04 \times 10^{-6}$$

$$g_e = 9.780,368\ (1 + g' + 1/3\ f')\ \text{m/sec}^2$$

$$g' = 0 \pm 3 \times 10^{-6}$$

$$k_e = 1.197,918,5\ (1 + a' + 1/2\ g' - f')\ \text{megameters}^{3/2}/\text{min}$$

$$k_e = 0.074,365,74\ (1 - 1/2\ a' + 1/2\ g' - f')\ \text{q radii}^{3/2}/\text{min}$$

$$k_e = 0.074,365,74\ (\text{no uncertainty})\ \text{g radii}^{3/2}/\text{min}$$

$$\frac{\Delta k_e}{k_e} = \pm 11 \times 10^{-6}$$

In this presentation the uncertainty goes into the equatorial radius, a_e.

$$a_e = 1\ \text{q radius} = (1 + 1/3\ a' - 1/3\ g' + 2/3\ f')\ \text{g radii} \pm 4 \times 10^{-6}$$

*Herrick, S., R. M. L. Baker, Jr., and G. Hilton, U.C.L.A. Astronomical Papers, Vol. 1, No. 24, 1957, pp. 297-338.

Question Doesn't your use of the term "g radius" cause confusion since this term is used in relativity theory for the gravitational radius?

Herrick Yes, it should be called something else. Now that we have artificial earth satellites, we can determine J from observations. The advantage of adopting a value of k_e is that we readjust the results of integration of the motion of satellites as new data are available rather than repeating the integration.

A more familiar terminology is:

$$GM = k_e^2 = 3.986,135,3\ (1 + 2a' + g' + 2f')\ x\ 10^{-4}\ \text{megameters}^3/\text{sec}^2$$

Satellite observations which are now available give a value for the flattening of $f = 1/298.3$. In this case, f' has a definite value and $a_e = 6\ 378\ 145$. I agree that now the preference is for the second rather than the minute, which is used in the former expression. In addition, the number for a_e used in this original paper resulted from the work of the Army Map Service and Irene Fischer. It was a reduction based on areas rather than arcs, and perhaps she can now discuss this work.

Fischer In general, geodesists do not attach as much meaning to the number used for the equatorial radius as we here would like. But one becomes bogged down with its meaning. There may be a discrepancy in the interpretation by astronomers and geodesists of the concept of the equatorial radius. In rethinking the concept of the earth's equatorial radius, we should consider the change of this concept in the past. In the "golden days", one needed only one determination to know the size of the earth. Eratosthenes made such a determination of the earth's radius, and this then could be used for a scale. Later, when refinement of an ellipsoid of revolution was adopted, two parameters were needed: the equatorial radius and the flattening. This was good enough as long as the underlying philosophy regarded the ellipsoid of revolution as the actual shape of the earth. Today, it is realized that the geoid is a very irregular surface, and its mathematical description is difficult. If the geoid were known, it would just be a question of finding the best mathematical representation. The reference ellipsoid plus the departures give the geoid. In general, it is the departures which are treated mathematically.

The geoid is not globally known. In my paper,* Fig. 5 (p. 258) shows a map giving the coverage of triangulation data of the world. One can see how spotty the coverage is, and therefore it must be realized that the present ellipsoidal figure of the earth is an extrapolation of limited data. The coverage by gravimetric data is just as poor. If we try to pin down what the reference ellipsoid really means, we find it is the best representation of the available data. If the earth were really an ellipsoid, each surveyed arc would yield a true representation. In the Jeffreys' papers** all of these arcs are listed. The underlying philosophy here is that each arc _is_ a part of the same thing. However, today we know each local region fits to a _different_ ellipsoid of revolution. In North America, the data fit to something different from that in India. The question arises as to how all local-area-ellipsoids fit together? They may not even be concentric.

It was for this reason that I changed from the concept of arcs to the concept of areas and as a first step computed the North American geoid. The map is reduced in my paper, "A Map of Geoidal Contours in North America."*** I have also done this for Japan, Manchuria and Korea. Bomford has done the same for India and Lieberman for Central Europe. By now several pieces exist, tied together within each hemisphere. There is no geoidal connection yet across the ocean, therefore a best-fitting ellipsoid for both hemispheres can be derived only by assumptions across oceans where data do not as yet exist. The different numbers resulting for a_e

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*"The Present Extent of the Astro-Geodetic Geoid and the Geoid World Datum Derived From It," _Bulletin Geodesique_, New Series, No. 61, Sept. 1, 1961.

**"The Determination of the Earth's Gravitational Field," _Mon. Not. Roy. Astron. Soc. Geophys. Supp._, Vol. 5, 1941, pp. 1-22, and Vol. 5, 1943, pp. 55-66.

***_Bulletin Geodesique_, No. 56, 1960.

reflect these assumptions.

We thus have a set of local best-fitting ellipsoids. It had been thought that more data and better fitting would result in a series of ellipsoids that would converge to the truth. This is not really so. Any reasonable numbers for ellipsoid parameters can be found in studies of the past 150 years. While it is true that the best ellipsoid is defined by all the local geoid pieces, we seek now primarily this geoid, that is to enlarge the knowledge of the earth's shape. To fit an ellipsoidal figure of the earth to the geoid is a secondary goal now; before, it was the primary one.

The difficulty with asking for the best equatorial radius is that a world datum is not defined by this one parameter; but by five parameters. Of these five parameters, a, the radius, and f, the flattening, describe the shape of the earth; the three additional parameters, ϕ, latitude; λ, longitude; and h, geoidal height of a reference point, give the relative position between ellipsoid and geoid. One parameter out of five doesn't tell much. It is here again that the philosophy changes. The ellipsoid of revolution for the earth has the role of a coordinate system. (The convention in geodesy is that the small axis is parallel to the axis of rotation--it could well be the other way, i.e., by a simple rotation in the coordinate system. Geodesists use ϕ and λ and h because they are used to it.) The connection between ϕ, λ, h and x, y, z is merely a connection between coordinate systems. In astro-geodetic work, we don't know the relation of the center of the earth to the standard datum point, Meades Ranch--it may be off as much as 200 meters from the origin of the coordinate system.

Clemence There are two distinct astronomical requirements for the shape of the earth. First, the model of the earth should be as simple as possible for dynamical reasons (the de Sitter system, for example, which uses four constants) and secondly, we need the actual x, y, z coordinates of specific stations with respect to the center of the earth. We really don't need to know the last two figures of the equatorial radius.

Herget Increasing the rms departures in fitting an ellipsoid to the geoid does not imply departures from a "true" ellipsoid, as is sometimes implied.

Kaula The only reason astronomers need an accurate value of a_e, the earth's equatorial radius, is to obtain a better value of the gravitational constants. Geometric positions relative to the center of mass are needed by astronomers together with parameters expressing the gravity field. Gravimetry integrated over the earth's surface gives a geoid surface. Astro-geodetic methods give the shape of level surfaces which are projected to the ellipsoid to define another geoid. It is necessary to get a_e from astro-geodetic data. Gravity anomalies of continental dimensions are equivalent to the low degree harmonics. The coefficients of J_n drop off rapidly with n. This indicates sources for harmonics which are deep in the earth.

I agree that the ellipsoid is an arbitrary coordinate system; however, we can express the ellipsoid of revolution by a fewer number of parameters than any other usable reference surface. The proper reference figure is one that with the fewest parameters can explain the shape of the earth from which the departures from the actual surface can be represented by small linear terms. This would be a two-element ellipsoid using only a_e and f. Departures (e.g., triaxiality) can be treated as perturbations.

Question But tracking station locations require positions relative to the mass center.

Kaula Corrections for the geoid-ellipsoid discrepancy go into determination of position computed through geodetic triangulation.

Herrick Astronomers are interested in a_e as the unit of distance. They are also interested in x, y, z, and in the harmonics, the J's, which are of interest in f.

Kaula GM_e and J's are used for description of the figure. The relation between GM_e and a_e depends on the mean value of gravity over the equipotential surface to which a_e pertains.

Brouwer There is also a need for the relation between a_e and the a.u.

Kaula a_e can be expressed in any system of units.

Herrick The expression of a_e in terms of a g radius fixes GM.

Kaula We can also fix a_e and let other constants vary in accordance with these fixed relations.

Brouwer We should remember the geoid corresponds to the mean sea level.

Herrick Astrodynamic uses of geodetic and geophysical constants should be emphasized. What is a sufficiently good a_e for astrodynamic purposes?

Question Is the time ripe to adopt a Gaussian constant for the earth? And if so, what is the value of a_e to be used for this type of treatment?

de Vaucouleurs The constants that are needed depend on the user. The astronomer is not necessarily interested in the a_e, while it may be required for, say, tracking satellites. Most a.u. determinations give the distance, not the solar parallax. Lunar parallax requires the Greenwich-Cape arc. I agree with Brouwer that the geoid corresponds to the mean sea level, but this is difficult to determine.

Clemence It is a computational convenience to have an ellipsoid with a_e in meters, and elevation h, and the x, y, z, coordinates. Astronomers do not care about the philosophical problem. They want this ellipsoid for astronomical purposes--that it, for the calculation of parallax, etc.

Hunt Terms such as GM, and the harmonic terms, J's, may be improved by U.S. Air Force programs. Reference should be made to Kaula's report,* which gives the NASA values of constants. Various programs now in use would benefit by a standard list of constants.

Discussion At this time a joint discussion began on the need for standards in order to reduce observations and to be able to compare results. Irene Fischer reported that Bomford was proposing to the AGU the adoption of $f = 1/298.3$ and $a_e = 6378\ 155$. The derivation resulting in 155 is, in his opinion, more correct than the others. Values in use at present are: 155 (South Asia), 166 (Mercury

*Kaula, W. A., "A Geoid and World Geodetic System Based on a Combination of Gravimetric, Astro-Geodetic, and Satellite Data," NASA Technical Note, D-702, May 1961.

and Apollo projects), 165 (political agreement, Kaula and NASA), 165 (Goddard), and 150 (the Russians). The reduction of the Yaplee radar determinations of the moon's distance yields 165, depending on what one uses for the mass of the moon.[7] Fischer again pointed out that the adoption of a specific number is an adoption of a particular reduction.

At this time, A. Wilson called for a working session on "the equatorial radius, a_e, and the question of a Gaussian constant for the earth."

WORKING GROUP SESSION
Friday Afternoon, 2-23-62
(R. Moore, Chairman)

IV. SPACE EXPERIMENTS

Moore suggested that the working group address itself to the problems raised in the symposium so far by considering 5 categories:

1. space flight experiments
2. lunar orbits
3. how best to evaluate raw data
4. potentialities of ground-based radar, balloon, and surface astronomy
5. accuracy needs of users

Moore briefly discussed the present NASA lunar and planetary exploration programs. Specifically with regard to the lunar program, there will be a series of seven Rangers that will carry television cameras to impact on the moon. This will be followed by a series of Surveyor A that will soft land on the moon and transmit TV pictures of several types. This will be followed by a series of Surveyor B. This will be the same spacecraft as the Surveyor A series, but there are tentative plans that at least one, maybe more, will be lunar orbiters at 200 to 400 km above the moon's surface.

With regard to the planetary program, there will be two Mariner R shots to Venus in 1962, and there are planned Mariner B shots to Venus and Mars in 1964. (Full notes of their instrumentation are available in the literature distributed widely by NASA.)

A series of questions and discussions followed concerning the effect of the earth on a lunar orbiter. It was discussed in detail whether it will be possible to obtain mass as well as mass distribution, i.e., triaxiality information about the moon from an orbiter. It was the consensus that many orbits will be needed to get this information. Davis questioned whether angles could be measured accurately enough from surface tracking from the earth and suggested that the angles should be measured from the orbiting spacecraft. Hunt suggested that there should be a transponder on the lunar surface as well as in the orbiter. In this way, the required collection of months and months of orbital data to obtain the necessary accuracy would be reduced. The group then discussed whether radar could be put aboard a lunar orbiter. It was agreed that one planetary flyby could not give data of any great value with regard to masses of the planets. However, uncertainties are still so great that, with some luck and very good orbital tracking, we might be able to get something from flybys. De Vaucouleurs gave a detailed discussion of the present knowledge and uncertainties with regard to the diameters and masses of Mercury, Venus, Earth, and Mars.

After the coffee break Moore gave some more details on the Mariner B program. It was concluded that from this original Mariner B program all we can expect are transponder data and therefore another experiment in the determination of the value of the a.u. in terms of the STL experiments. There was a brief discussion on the geodetic satellite by Gabler and Greenfield. Schilling raised the question of an artificial planet, and what kind of data a transponder could give us if it were working for a period of a year or so. De Vaucouleurs mentioned that an Earth-based balloon program could help in the tracking of space probes with modest-sized reflectors. Hunt mentioned and discussed the GRD balloon program with the first flight scheduled for March. Davis raised the problem of determining the a.u. by radio methods. He referred to dispersion and the possibility of different electromagnetic wave velocities. He suggested that one should attempt to track space probes simultaneously at two frequencies. De Vaucouleurs suggested that a flyby probe could give us an exact determination of diameter if we could observe and time the radio occultation of the probe behind the planet. However, Schilling pointed out that knowledge of the flyby probe's orbit is not accurate enough to make the occultation observation meaningful. Smith suggested that perhaps this could be tried out on the moon. We would need two surface stations here on the earth spread far apart in horizontal distance. In summary, the general conclusion was that, as far as uncertainties in solar system constants are concerned, we would not learn too much from space probes in the next two to four years.

WORKING GROUP SESSION
Friday Afternoon, 2-23-62
(S. Herrick, Chairman)

V. EARTH'S EQUATORIAL RADIUS

Since the discussion of geodetic and geophysical constants (Session III) resulted in disagreement regarding the use of the unit of distance to characterize the earth's radius, as well as the expressions and theory which define this quantity, the problem was delegated to this working group.[8]

Herrick In discussing the advantages of treating satellite orbit determinations by a process which sets $k_e^2 = GM = \text{constant}$, it is necessary to consider the integration of the equations of motion. From the expression,

$$\ddot{x} = -k^2 \frac{x}{r^3} + \ldots\ldots \text{ (higher order terms)}$$

the problem of relating this equation to an observation is seen in the integration:

$$x = x_o + \dot{x}_o \tau + \iint_o^\tau \ddot{x}\, d\tau^2$$

where $\xi = x + X$ and X, the position of the observer, is: a cos θ cos ϕ. If at any point we have to introduce new data, there is no change in k but rather in X.

Question If the position of the observer were fixed, then new data would require a change in k?

Herrick Yes.

Question Why not hold the time conversion τ fixed?

Herrick But $\tau = k\ (t - t_o)$. So if you hold τ fixed and stay in real time, you do the same thing by holding k constant.

Kaula I don't think units should change.

Herrick They don't; only a conversion factor changes.

Kaula How do you keep them from changing?

Herrick Say, for observer at $X = a_e \cos \phi \cos \theta$, and, since $\xi = x + X$, by adjusting x we can even keep X the same.

Kaula But doesn't this mean that you have to change the time if we keep the integration in time the same?

Herrick No.

Kaula Suppose on different passes you have

	δt
ξ_1	0.00
ξ_2	.01
ξ_3	.02

you say that we can change X to eliminate δt. If you do the station position correction, $x = \Delta x + \Delta Lx$ becomes equivalent to a length conversion change in t.

Herrick We can also change initial conditions. We correct orbits by a change in initial condition, but we keep the integration the same for the same initial condition.

Kaula The work looks the same. Partial derivatives will always be needed to express the effect of corrections, including those to initial conditions, on the satellite position.

Herrick We do shift in the integral but not in the integrand.

Kaula and Kozai For a satellite, since the changes are great (due to drag, etc.), a consistent integration is not necessary, and the method of calculation doesn't matter.

Herrick and Baker For higher orbits this does matter. Our method is easier.

Herrick If we can avoid reintegration, we don't lose anything, and we can make comparisons between results of integrations.

Kaula All we need is a standard system of conversion between the astronomical system and the cgs system.

Herrick But all that we say is that whatever conversion you make, it can be done in the last stages. We don't lose anything, and we can preserve the integrations. We can even keep observer positions fixed.

D. Wilson Persons not conversant with astronomical practice do not understand this method of adjustment.

Herget Yes, Bill evidently thought that we had to change the station positions. This isn't true. We can change a_e slightly and preserve these.

Herrick We can change units of measurements.

Kaula From your paper it sounds like you abolish uncertainty in distance, but you really just shift it.

Herrick Right. We fix k_e, changing g-radius in terms of q-radius (see Herrick, Baker, and Hilton paper). Thus we transfer uncertainty to a_e.

Kaula You mean that in the expression:

$$n^2 a^3 = \mu$$

you adjust the a to fit observations? The phraseology of your paper sounded like you abolished uncertainty in observations.

Herget Well, what terms should be used, Bill? Astronomers think they know what they're saying, but to avoid semantic troubles they need help.

Herrick The use of the q-radius gives zero uncertainty in a_e. Use of g-radius gives zero uncertainty in k_e.

Herget This really is a problem of clarifying concepts without inventing bizarre nomenclatures.

Fischer How about new words for these concepts?

Herrick How about "geocentric unit" (g.u.)? The gravitational radius is used in relativity theory, as Clemence mentioned.

Fischer Radius is a poor word here since it implies size. Geodetic unit doesn't seem right, "centric" may be better.

Baker That would seem to require a change to heliocentric a.u.

Herrick Call it the g.a.u. implying h.a.u.

D. Wilson You could define the term and merely suggest terminology.

Herget Geo-unit seems best.

Herrick Geo-unit seems appropriate as an abbreviation of geocentric astronomical unit. We agree then. These are the values of k_e suggested by the Herrick, Baker, and Hilton paper. There we set k_s and rounded off k_s^2. Gauss gave

$$k_s = 0.017,202,09895$$

If we round off, we change a_e in geo-units, making the difference from unit greater.

Baker For qualitative work it is good to have $a_e \cong 1$ in geo-units.

Herrick Let's compute f' and a'. What is the status on g' currently? Should the value of k_e be revised to the NASA value? Using a' and f', 1 geo unit = $(1 + 9 \times 10^{-6})\, a_e$. We can revise it to unity by changing k_e.

Baker Should we do this? If so, now? Do geodesists care about this?

Fischer No.

Herrick If our revisions change other quantities, we can modify k_e.

Kaula How about k_e of the DOD system?

Herrick There is no necessity of changing k_e in lab units. We only care about geo-unit case. How about the J_4 contribution to k_e? We (Herrick, Baker, and Hilton) used x = 0.

Kaula Our k_e is consistent for an ellipsoid of revolution (a_e + f specified). It does not depend on J_4.

Herrick It seems the J_n terms should affect k_e.

Kaula We give absolute J_n's and k_e depends only on the reference ellipsoid. The DOD system gives the geometric ellipsoid which implies J_4, and hence x = 0. J_4 affects $a_e - a_{mean}$.

Herrick This doesn't affect the end result, does it? We can discuss this at length later; whether a_e or a_{mean} is used doesn't matter.

Kaula The reference ellipsoid is an ellipsoid of revolution. Given a_{mean} the computation of a_e depends on whether you have J_2 or J_2 and J_4. Geodesists use a_e as the equatorial radius of ellipsoid of resolution, and hence it does not depend on J_4.

Herrick So, for the geometric figure we can get k_e using any radius (a_e, a_{mean}, etc.). We can certainly check this, perhaps by a committee composed of Herrick, Kaula, and Baker. The question is whether or not to do it now.

Kaula How about k_e = 1 and correct t.

Herrick $\tau = k\,(t - t_o)$.

Herget Seconds are standard in our observations. Observers use radio station WWV. We should reference published values and show how to get the geo-unit closer to unity. At least geo-unit should be closer to a_e than 9×10^{-6}. I would prefer putting out a unit value for geo-unit at the start.

Herrick I agree, we agree, and so we can recalculate k_e in consultation.

Herget Where does a' come from?

Baker/Herget From a change in f affecting a_e.

Herrick So we can use $f = 1/298.3$ in the recalculation.

Fischer We take that from the satellite results. We have best fit for the geoid from each hemisphere, and, with new data, a and f will change. The shape of the geoid is invariant, but we get different fits as data change. The new satellite data seem more definitive for f.

Herrick So we can take $f = 1/298.3$.

Herget What about ellipticity of the equator?

Kaula This is included in uncertainty of J_n's published by us. We still want to use an ellipsoid of revolution for reference. The triaxiality of the earth is small enough to be considered a first-order perturbation.

Herget So uncertainty due to J_2^2 will appear in f', a', etc.

Herrick We can adopt $k_e = 1.239445_0 \times 10^{-3}$ (geo-unit)$^{3/2}$/sec.

Herget We want to say that, if so and so is adopted, then such and such follows, and if such and such, etc. We don't want to adopt anything. Where would a report go?

A. Wilson We are not an official group, but we can make recommendations to other groups.

Herrick I can contact COSPAR on the possible formation of a committee. This could be set up by contacting Van de Hulst. I can keep some of you informed on this. A group of us can correspond. Bill, are your derivations available?

Kaula Yes, in our NASA tech note (Ref. 17) we give references.

Herrick We have (from Miss Olds) the computed figures.

$k_e = 1.239, 445, 24 \times 10^{-3}$

$k_e^2 = 1.536, 224, 50 \times 10^{-6}$

for $g' = 0$, $a' = 3.136 \times 10^{-6}$

$f^1 = 1.467 \times 10^{-5}$

$k_e^2 = 3.986, 043, 5 \times 10^{-6}$ -88

$(1 + 2a' + g' + 2f') = (1 - 22.8 \times 10^{-6})$

Now $g_e = 9.780368 (1 + g' + 1/3 f')$, where $g' = -20$

Kaula We get g = -14.0

Herrick Good. This would bring down the total variation in k_e^2.

A. Wilson Work through COSPAR seems appropriate.

Herrick, Herget, Kaula Discussion of COSPAR details, especially Russian cooperation, which seems desirable.

Herrick Is there anything else to consider before quitting?

A. Wilson Would you write out your views on any of this?

Herrick Doubt if I can, but I will keep in touch with COSPAR and Van de Hulst. These can be made tentative enough so that you can repudiate me if necessary.[9]

Herget We should be emphatic on settling this matter (k_e). Many don't realize the importance of the Gaussian method. We should emphasize this.

A. Wilson This can be recommended by passing a resolution. Actual numbers should be acted on by COSPAR.

Herget Programmers sometimes change these parameters indiscriminately. If COSPAR does act, we need to impress users, even if they don't understand the problems completely.

Wilson Is there any opposition to this kind of COSPAR action?

Herrick Not really. Clemence is willing to wait and see. He is also concerned about the effects on lunar tables. We discussed this at lunch with respect to the lunar parallax. Fixing k_e is really no different from fixing a_e, although Clemence may not be completely convinced.

Herget Does the Lunar Theory support this?

<u>Kaula</u> Not if you use the radar measurements of Yaplee. This ties the lunar motion to cgs.

<u>Herget</u> We still have angular motion, which is independent of cgs.

<u>Kozai</u> We will eventually have J_2 more accurately.

<u>Kaula</u> Possibly to 6 significant figures. J_2 refers to gravity field, not to the reference ellipsoid.

PLENARY SESSION
Saturday Morning, 2-24-62
(D. Brouwer, Chairman)

VI. LUNAR CONSTANTS

Brouwer As a guide for discussion, let us consider this morning the following subjects:

Lunar inequality (mass of the moon)
Libration
Watt's irregularities (moon's limb)
Eckert's verification
Distance: Yaplee's radar determination

First with regard to the lunar inequality--there are many observational difficulties encountered in seeking to obtain data on the mass of the moon. Following from the de Sitter/Brouwer paper (pp. 223-224) the dynamical parallax expressions:

$$\pi = \frac{\sin \pi_{☾}}{\sin 1''} \qquad \text{and} \qquad \pi_{☾}^3 = C\,\frac{R_1}{g_1\,(1+\mu)}$$

where

$$\mu = \frac{M_{☾}}{M_{\oplus}} \qquad 1 + \mu = \frac{M_{\oplus} + M_{☾}}{M_{\oplus}} \qquad \mu' = \frac{\mu}{1+\mu}$$

The expression for the precession constant, $P = (A + B\mu')H$, can be determined from observation; the constant H can be determined from theory and from the theory of hydrostatic equilibrium, $J = qH$.

Jackson and also Jeffreys have pointed ou the lack of rigidity of Earth. Too little of the earth's interior is known to speculate further.

From minor planet determinations at the time of a close approach, the uncertainty of the observed value of the lunar inequality, L, is greater than the uncertainty in the observed value of the solar parallax. The value of L from observations of Eros, for example, depends on observations over an arc of more than 30° in the sky while the solar parallax displacement is in a small area. Therefore, this determination depends on positions of stars over a large area, and the limit is the limit of determining star positions--approximately 1/10". Spencer Jones, and Hinks before him, used star positions along the path of Eros. They then made empirical corrections to the star positions. Delano used point of zero inequality points as normal points and made orbit corrections.

L ranges from μ^{-1}: 81.28 (Jeffreys)
81.30 (using Rabe's value of solar parallax)
81.37

The moon's mass enters into the relation between the distance to the moon, such as the recent radar determination by Yaplee: 384,400 ± 2 km, and the earth's equatorial radius. The radius of the moon is also involved.

Question How do you evaluate the use of a lunar satellite for determining the mass of the moon?

Brouwer If one measures the doppler data and the period data, one could get a dynamical determination.

Kozai Since the perturbations of the earth are large, one could get the mass of the moon from an Earth satellite.

Question What are minor planet possibilities?

Brouwer Not too exciting, since the total displacement is 6". Betullia covers a large area of sky, and as explained above for Eros, the determination depends on positions of stars over a large area.

Hunt In an in-house plus contractual program supported by GRD, we are attempting to obtain a better determination of the geometrical and mechanical figures of the Moon through the acquisition, measuring, and reduction of high-resolution lunar photographic materials. We are also considering the re-determination of the Moon's physical libration constants (λ, β, h, f, I) utilizing available past heliometer observations plus the use of modern high speed electronic computers. In this connection, Prof. K. Koziel, President of Commission 17 of the IAU, is performing a total reduction of heliometric series at the University of Manchester by invitation from Prof. Z. Kopal. It is expected that the preliminary results of this study should be available within the next six months.[10] The main difficulty is not in the theory but in the correction, re-computation and adjustment of individual heliometric series in order to arrive at one set of physical libration constants based on a combination of all available series of heliometer observations covering a time period of over 100 years. Until a time when a sufficient quantity of high-resolution selenodetic photography is acquired in order to resolve the problem by applying specialized photogrammetric techniques, the reduction of heliometer observations will remain as the best source of lunar physical libration data.[11] Finally, on the subject of the mechanical ellipticity of the Moon, an informative review is discussed in Arthur's paper, with which some of you are familiar.*

<u>Clemence</u> <u>A review of Watts' study of the Moon's limb</u>

There are three techniques of optical observation of the Moon's position, all of which are subject to systematic errors due to irregularities in the limb:

1. Meridian circle observations
2. Occultations of the Moon by stars
3. Photographic techniques--i.e., photographing the Moon against the background of stars

In the 18th century Hayn discussed the correction of these observations, but in recent years Watts has repeated this work in a more exhaustive manner and finds improvements. This work will appear soon, in the form of some 1800 charts which give any point, which can be observed because of librations, reduced to a datum surface. Mrs. Sadler at the Royal Greenwich Observatory has recomputed some of the occultation data based on these new corrections.

It now seems clear that the divergence between the center of mass and center of figure is due to mountainous areas of the southern limb and the fact that observers have set micrometer wires at tops of mountains rather than the base. This perhaps can be thought of as an optical illusion. That is to say, the figure of the moon is much nearer to a sea level figure.

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*Arthur, D. W. G., "Lunar Cartography and Photogrammetry," <u>Photogram Record</u>, British Phtogrammetry Society, November 1960.

Review of Eckert's verification of Brown's lunar theory, which is in progress, is a numerical verification of Brown's theory. If one could get a theoretical value of the motion of perigee and motion of the node, then one could compare observations of A, B, and C. The theory has not been sufficient up to now. The coefficients of the periodic terms will also be improved.

PLENARY SESSION
Saturday Morning, 2-24-62
(G. de Vaucouleurs, Chairman)

VII. PLANETARY CONSTANTS

Items to be discussed:

- planetary masses
- planetary diameters
- satellites of Mars
- pole of Mars
- rotation of Venus

de Vaucouleurs The current values for diameters are:

Mercury: $6''.46 \pm 0''.03$ (prior to 1960)
$6''.65 - 6''.72$ (transit of Nov. 1960)
$6''.68$ (ephemeris)

Venus: $16''.82$ (ephemeris)
$16''.86 \pm 0''.02$ (1962 revision)

Mars: $9''.41 \pm 0''.01$ or $0''.02$ (equatorial, 1962 revision)
$9''.36$ (ephemeris)
flattening: 0.010 optical
0.0052 dynamical

The discrepancy in the values of the flattening for Mars still presents a difficulty. The weight of the optical determinations cannot be ignored.[12]

Kaula There is a recent paper by Lamar on the optical versus dynamical flattening.*

Kern MacDonald's work attempts to account for the two flattenings by invoking a change in composition and density of the material in Mars at a depth of about 1200 km. Since the planet is rotating rapidly, the pressure at a given depth is lower near the equator than near the poles. The outer shell of less dense material will therefore be thicker near the equator than near the poles. Thus the equatorial radius will be greater than the polar radius and

*Lamar, D. L., Optical Ellipticity and Internal Structure of Mars, The RAND Corporation, RM-3127-JPL, June 1962.

the optical flattening greater than the dynamical ellipticity.[13]

Discussion With regard to planetary masses, Brouwer and Clemence review this data in Vol. 3 of The Solar System.* Mary Francis of UCLA discussed her program to obtain masses of Venus and Mercury from the reduction of Icarus data. This data includes 50-100 observations made in 6 different years over a period of 10 years.

Reference was also made to Makarowa's work (Leningrad) in determining the mass of Mercury from Encke's Comet.** His value is in close agreement with that of Rabe.

Question What is the possibility of improving the mass determination by the future Mariner flights?

Clemence I have little confidence that one-way trips such as these will afford new data. The better data will come from vehicles which orbit the planet. On the subject of the satellites of Mars, little improvement can be expected in the determination of the mass of Mars until additional observations are made (over a period of approximately 10 years). The supposed acceleration of the inner satellites (e.g., Phobos) as discussed by Sharpless*** is based on a false premise.

At the present time, Wilkins of the Royal Greenwich Observatory is reducing plates made by Kuiper, which will add weight to the mass determination of Mars. To date the only conclusion is that there is no evidence for any acceleration.

A year ago a Russian astronomer, I. S. Sliklovskiy,**** concluded that satellites were hollow (artificial?) based on the assumption that this acceleration existed.

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*Planets and Satellites, Vol. 3, The Solar System, G. P. Kuiper and B. M. Middlehurst (eds.), University of Chicago Press, 1961, pp. 57-64.

**Bulletin of Theoretical Institute, Vol. 7, 1958, pp. 1-18.

***Sharpless, B. P., "Secular Accelerations in the Longitudes of the Satellites of Mars," Astron. J., 51 (7), 1945, pp. 185-186.

****Foreign Technology Division Translation, WP-AFB, FTD-TT-62-488, May 1962.

de Vaucouleurs On the subject of the pole of Mars, for the needs of map projects such as the Harvard-Texas current work, the celestial coordinates of the pole of Mars need to be corrected by at least 1°. The work of Trumpler, Camichel, Burton, and others disagrees with the standard values used in the physical ephemeris published by the Nautical Almanac.

Discussion On the subject of the positional accuracy of the ephemeris of Mars, Clemence reported that a provisional ephemeris to the year 2000 exists in the Naval Observatory Circular with an error of about 2 per cent of the diameter of Mars. When Duncombe's work is finished, the error will be reduced to approximately 1/2 per cent.

On the subject of rotation of Venus, the JPL work was reviewed by Gates. Narrow doppler evidence suggests a slow retrograde rotation rate for Venus.[14]

PLENARY SESSION
Sunday Morning, 2-25-62
(R. Baker, Chairman)

VIII. ASTRONOMICAL UNIT DETERMINATIONS

Clemence There are three principal methods of determining the solar parallax that are more precise than any others; the dynamical method, the radar method, and the geometrical method: such as the determination of the parallax of Eros (same principle as determining the parallax of a star).

Rabe's value* is based on the dynamical method, and at the time there was conflict with Spencer Jones' determination by the geometrical method. I recall that I discussed with Rabe various possible reasons for the discrepancy:

1. It is known that the constant of precession, P, requires a correction of about 0".8/century. The effect of using an erroneous value gives a spurious motion to the perihelion or the node of the planet. This effect was real in Rabe's work; however, after further investigation it was found that the effect had no result on the final value of the solar parallax.

2. The question of relativistic motion of the perihelion was omitted in the Eros determination. Rabe and I agreed, however, that it was not necessary to repeat the calculations to see that this effect does not enter the determination.

3. Finally, Atkinson decided to determine the parallax of a star using the same telescope that had been used in Jones' geometrical determination. The method is different for observing a star than for observing a planet (i.e., for a star, the a.u. is the base line and for a planet, Earth is the base line). Therefore, for a single planet, it is necessary to make the observations well off the meridian, and for a star, near the meridian. Atkinson's work resulted in a negative parallax for a star determination, which showed that the optical determinations might all be affected by systematic errors inherent in the instrument. However, since

*Rabe, Eugene, "Derivation of Fundamental Astronomical Constants from the Observations of Eros During 1926-1945," Astron. J., 50 (1184), 1950, pp. 112-126.

there is no certainty that optical errors remain constant with time (30 years), there is no way to apply a correction. I therefore feel that the optical parallax determinations are not valid.

Question What then, if any, is the usefulness of optical determinations?

Clemence If concurrent studies of the optical system, i.e., comparing a star to a planet are made, there is a great deal to be gained. De Vaucouleurs mentioned that James Baker (Harvard) is currently experimenting on this problem.

Question Where do errors arise in optical determinations other than those which are treated in the classical methods of determination of the plate constants?

Clemence The difficulty lies in the assumption that the image of Eros on the plate has the same relation to the images of the background of the stars, which is not so. The telescope displaces the center of field, and in the plate-constant reduction this is not removed. If comparison stars were sufficiently close to Eros this might be overcome.

Herget Even if Baker's work is successful, the increase in the accuracy of optical determination will yield only a small improvement in the determination, while on the other hand the dynamical method will yield greater improvement because of the availability of machine computations.

On the subject of rework of Rabe's determination, I feel that Rabe is still interested in the project, but the question is what catalog of stars to use. Clemence does not feel it is necessary to wait for the AGK-3, since it would not improve star positions except for proper motions; and the FK-4, which is available now, would work.

Question What is the probable error of the results if this work is repeated?

Herget Rabe'e method is essentially the determination of the mass of the earth/moon ratio. The solar parallax is inferred from this by the use of de Sitter's formula. The plot of the perturbations of Eros over a 30-year interval resembles the synodic period of Eros with respect to the earth. The procedure is first to calculate the perturbations using a mass of the earth, and second, to set up a correction to the orbit of Eros plus the earth in the equations of condition. Rabe included the masses of Mercury, Venus, and Mars, i.e., 16 unknowns: six elements of the orbit, four unknown masses, and six other unknowns in order to obtain a differential correction in the least squares solution. (See page 117 of Ref. 30)

It remains to be settled whether or not the extent of the work will yield a great enough improvement in accuracy to warrant a re-work.

<u>de Vaucouleurs</u> Concerning the philosophy underlying an objective approach to this question, I should like to stress the value of the treatment of Harkness (Ref. 9), i.e., it is important to give recognition and proper weight to all methods. Each method may, and generally does, have unknown systematic errors; as the long history of the solar parallax proves. It is generally a mistake to say at any given time that this method is wrong, but this other method must be right. By giving all the weight to one method and zero weight to all others the benefit of statistical cancellations of the (unknown) systematic errors is lost.

PLENARY SESSION
Sunday Morning, 2-25-62
(W. Kaula, Chairman)

IX. STATISTICAL CONCEPTS

Kaula There is no statistical wand to substitute for knowledge of subject matter. The methods I will discuss here have been developed for close satellite orbits and terrestrial geodesy and are not treated in standard textbooks. In general, there are two places where standard statistical theory falls down, and the applications that I want to discuss are examples of these: (1) systematic errors that are a result of the incompleteness of the mathematical model, and (2) the non-uniformity of distribution of observations. There are other sources of systematic errors, such as those in star catalogs, but these I will not consider here. We can think of systematic errors as equivalent to the off-diagonal terms of the covariance matrix. This discussion follows the treatment given by Brown in 1955* and by Kaula and Fischer in 1959.**

We let x represent a column matrix (n x 1) or column vector of corrections to observations and z represent a column matrix (p x 1) or column vector of corrections to parameters. In terms of these, the condition equations in matrix form are represented as:

$$Cx + Mz = F$$

when C is an [m x n] matrix, M is an [m x p] matrix, and F is an [m x 1] matrix. With the expression for the "generalized" least squares condition,

$$R = X^t \, W^{-1} \, x = \text{minimum}$$

we have in matrix notation a statement of the problem. If the simultaneous equations were written out in detail, i.e., by expanding the matrix expressions, one familiar with differential

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*Brown, D. C., A Matrix Treatment of the General Problem of Least Squares Considering Correlated Observations, Ballistic Research Laboratories, Report 937, May 1955.

**Kaula, W. M., and Irene Fischer, U.S. Army World Geodetic System 1959, Part I, Methods, Army Map Service Technical Report 27, November 1959.

correction procedures in orbit determinations or with the least-squares adjustment of data would recognize a familiar procedure except perhaps for one item. This is the nature of the $[n \times n]W^{-1}$ matrix. This could, because of its effect, be called a weight matrix, and in the usual least-squares adjustment it is a diagonal matrix:

$$W^{-1} = \begin{bmatrix} \frac{1}{\sigma_1^2} & 0 & 0 \\ 0 & \frac{1}{\sigma_2^2} & 0 \\ 0 & 0 & \frac{1}{\sigma_3^2} \end{bmatrix}$$

It is determined a priori from a knowledge of the measuring instrument and measuring methods. Investigators and writers on least squares methods, such as Brown, use the modification "general" least squares to indicate that correlation between observations is considered in determining the W matrix (variance-convariance matrix) so that it becomes more general,

$$W = \begin{bmatrix} \sigma_{11}^2 & \sigma_{12} & \sigma_{13} \\ \sigma_{21} & \sigma_{22}^2 & \sigma_{23} \\ \sigma_{31} & \sigma_{32} & \sigma_{33}^2 \end{bmatrix}$$

The off-diagonal terms in the W matrix can arise in a tracking instrument such as a radar, through the smoothing characteristics of the servo device where the serial or autocorrelation determines the relation between readings, or it can occur because coordinate transformations from actual observations to pseudo-observations are more convenient to use in the mathematical formulation of the problem.

To illustrate the general situation, for transformations, we can introduce the metric tensor of n dimensional space through

$$ds^2 = g_{ij}\, dx^i\, dx^j$$

$$= g_{11}\, dx^1\, dx^1 + g_{22}\, dx^2\, dx^2 + 2g_{12}\, dx^1\, dx^2$$

for $n = 2$, since $g_{ij} = g_{ji}$. The generalized least squares can be considered as finding the hypersurface for which ds^2 is minimized, and the g_{ij} as equivalent to the elements of the inverted covariance matrix.[15]

PLENARY SESSION
Sunday Afternoon, 2-25-62
(G. Clemence, Chairman)

X. SUMMARY SESSION

Clemence Let us continue the discussion on systematic errors which Bill Kaula started this morning.

A. Wilson On this subject, there is a paper by Yonden* of the National Bureau of Standards, in which the author suggests that a better way to isolate systematic errors is to change variables simultaneously rather than one at a time. I think this treatment is of interest to our subject of astronomical constants.

Schilling In addition, I would also like to raise the philosophical question of reality versus mathematical models. The point is that one can never prove that something is not true by models--one can merely approach the truth by models.

Clemence Yes, but in dynamical astronomy, the models seem to be so good. I agree, however, that mathematical models are good only to a certain degree. There is always the possibility that we cannot determine a single number for the solar parallax. However, as reluctant as we are to find the best way to proceed at the present time, there may be a fairly simple reason for the disagreement of the different determinations. Further experiments are needed.

A. Wilson I think the possibility exists for operationally defining these quantities in several ways. There may be more than one applicable model. Kaula's methods allow for discrepancies between the real-world models and observations.

Clemence An example of such discrepancies is that in the present ephemerides there is the possibility of a mistake of $0''.5$ in the longitude of Venus.[16]

de Vaucouleurs It is important to use all the information rather than selecting only one piece at a time. By using Harkness' method (Ref. 9) we can use all the information.

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*Yonden, W. J., "Systematic Errors in Physical Constants," Physics Today, September 1961, pp. 32-43.

A. Wilson There is the question of selecting a best value. How does the Harkness method allow for this?

de Vaucouleurs In principle, by a general least squares adjustment of all known relations and observations at the time; in detail, we would need to revise and re-investigate the basis for the relations he used.

Clarke Even so, it is still possible that a subjective element is still contained in it and that certain data should be rejected.

de Vaucouleurs In that case you may have to reject all data so long as you do not know for certain which are more precise.

A. Wilson You do not necessarily remove the subjective element, but a weighting procedure allows you to use values from many sources without knowing the individual details of each measurement. However, we need an objective opinion on the weighting procedure.

Clemence I agree. We all have our own philosophy on the presence of systematic errors. However, it is not necessarily true that one can find the sources of systematic errors by digging deeper. When systematic errors are known to be present, you find yourself in one of two positions: (1) you can continue to experiment and locate them, or (2) you can know of their existence but cannot determine their source. If systematic errors are known to exist but cannot be determined, we must try to reduce their weight to a negligible quantity.

de Vaucouleurs I do not entirely agree. We cannot always wait another year.

Clemence What is the value (worth) of a value for the solar parallax which is in disagreement between the one determined by Spencer Jones and the one determined by JPL--which is the most valuable? I would reject all optical determinations of the solar parallax in view of the work by Atkinson, who showed that systematic errors exist in the optical determinations.[17]

de Vaucouleurs Spectroscopic measurements are another independent optical technique.

Clemence I am not advocating reliance on one determination alone.

Schilling What are the real needs of the users in terms of accuracy of any individual value--let us ask them. That is, could you give specific extreme limits for the solar parallax--something better than a lower limit of one earth radius and an upper limit of infinity?

Clemence Well if you press me, my choice of the outside limits would be 8".78 to 8".81. We should however discuss the needs for an up-to-date system and the demands of the users.

de Vaucouleurs How about a small group to up-date the Harkness approach?

Clarke JPL is not concerned with the sequence of determinations but with the recording of observations. To improve the orbits of planets, we would have to compare with observations extending back to two centuries.

Clemence That may be possible in the case of planets, but not easy. In the case of Mars we did go back--we compared all the data since 1750 with the theory. This was possible only because of modern computing machines. On the subject of an up-dating system--there is the possibility that in 1964 the IAU will adop a new system which would be above all--self-consistent. That is, the theoretical relationships will be preseved and will remain in use for ephemeris purposes. Whether that will meet the needs of all the users appears to be doubtful. It is for the users to say they can or cannot wait that long. Those users who feel the need for an up-dating should come together and produce a system themselves.

A. Wilson Bob Baker mentioned this aspect of responsibility earlier; how could you proceed--especially with regard to the present U.S. space program?

Baker This depends on the users' requirements. The space program is really a pretty big user. As we have already heard here, there are tradeoffs between knowing the a.u. very accurately and the use of terminal guidance, for example.

A. Wilson Do we need a second official publication besides the almanac?

Baker I do not like the whole idea of several sets of ad hoc constants. We can have a series of adopted constants as consistent as possible, one series at one time in history, made in consultation with the users. For instance, I am opposed to putting down probable errors in the a.u. unless the users really need it. For instance, we heard about Mars-mapping. To what degree is accuracy really required? I think it will be a continuing process in which we should have several depositories.

Clemence I agree. An official system is only the basis of an almanac. No astronomer uses an official system. After all, this may be a temporary situation--only a few people are working in this field.

Wilson Let me repeat Baker's four criteria:

1. continuing
2. depository
3. dictated by user
4. consistent as possible

Block Why not use a single value of a given constant: for example, a single value of the earth's radius, a_e?

Clemence It is not possible to use one a_e without violating theory. Tampering with certain constants may have disastrous effects on the calculation of an ephemeris. This is a problem of consistency.

Baker As an example, let's look at the subject of physics. First-year students have the handbook of physics. A second-year student may use the nautical almanac; the third-year student will use the most recent value determined. Perhaps we have an educational problem. We can use at least two approaches: (1) a continuing effort for the users, (2) as a back-up basic theories with the relationships.

Mary Francis Referring to de Vaucouleurs' suggestion for a framework which relates the systems, it may be possible to have a set of astronautical constants more often than every 20 years. For instance, if you look at the different masses that are given in the nautical almanac--these are not at all current.

Clemence This may ruin us--we cannot reconstruct an emphemeris every year going back to the 1700's; we are stuck with the wrong values of the masses of the planets. If you want to introduce new planetary masses I will resist.

Hunt The Handbook of Geophysics, put out by GRD, will probably be revised next year. This revised edition will possibly include a chapter on planetary atmospheres, one on lunar and planetary geometry, as well as a section on astrodynamic constants. This publication will serve primarily as a users handbook, and perhaps could also serve some up-dating function regarding the standardization of a users list of astrodynamic constants.

de Vaucouleurs An up-dated handbook or table of constants is fine, but this does not remove the need for individual judgment in each specific case. For example, I have seen treatments of the ellipticity of Mars in which a mean of all kinds of optical and dynamical values was taken as the "best" value.

Discussion The discussion that followed bogged down on definitions as well as ideas on how to proceed in supplying values of solar system constants. In summary, the main ideas of the various proposals were:

1. The method developed by Harkness (Ref. 9) is powerful in that it can treat systematic errors that are thought to be present but whose sources are not known. It also permits feeding in new data when new observations lead to new results. (In essence, it makes a least-squares adjustment of all the constants simultaneously.)
2. Rather than a table of values, provide a matrix of tables of values, which gives the procedure of each method used to derive a particular table of values. In the future, by studying the various procedures from which particular solutions are reached, we may be able to derive a general, decision-making procedure for choosing values of constants.
3. Elect a set of superintendents to choose the values.
4. Elect a commission of users and experts to decide upon the values.
5. Establish a clearing-house where users can call in the experts for advice on specific problems.
6. More important than publishing the results of a particular study is the need to publish the values of the constants used.

PLENARY SESSION
Monday Morning, 2-26-62
(P. Herget, Chairman)

XI. SUMMARY SESSION CONTINUED

Hunt showed slides and discussed heliometer observations to determine the mechanical figure of the Moon. (See Section VI; also Refs. 13, 20.)

Fischer I would like to express my appreciation that this symposium refrained from adopting a standard system. Instead, it has distinguished between two needs. Because there are several different numerical values of the equatorial radius of the earth, the scatter only shows the uncertainty. In making the choice of a particular number, a particular user must consider the history of that number. I endorse the philosophy that we should be conscious of the process of choosing a number more than the number itself.

Clemence Review of de Sitter's paper on relativistic effects*

In this paper de Sitter gives all the formulae necessary (with the exception of typographical errors) to deduce all the consequences of general relativity on astronomical constants. There are two later papers in the Monthly Notices; however, in those he discusses cosmological consequences.

It is necessary to remember at all times that the equations of relativity have not been solved for more than one mass: either a revolving body of zero mass or a central body of point mass. (If the central mass is 100 times or more the revolving mass the results are valid.) The main formula (line element) is:

$$ds^2 = (1 + \alpha)\, dr^2 + (1 + \beta)\, r^2\, d\theta^2 + (1 + \gamma)\, c^2\, dt^2$$

where c is not the empirical value but a constant of the theory;
t is the time in the ordinary sense;
s is the proper time of a clock moving along this line;
r is the radius vector; and
θ is the longitude.

*Mon. Not. Roy. Astron. Soc., 1916, p. 699

If α, β, and $\gamma = 0$, we have the line element of special relativity; under the theory of general relativity, α, β, and γ have small values. We should not calculate motion of a body according to general relativity and later include conditions of special relativity (such as some writers have done).

If the universe is homogeneous and there is a spherically symmetric gravitational field, there exist only two sets of values of α and β. These two values signify two different methods of measuring the radius vector:

Under hypothesis A, $\beta = 0$, $-\alpha = \gamma = -\frac{2\lambda}{r}$

Under hypothesis B, $-\alpha = -\beta = \gamma = (-\frac{2\lambda}{r} + \frac{2\lambda^2}{2} + \ldots)$

λ is the gravitational radius of the central body and is

$$\lambda_{sun} = 1.48 \text{ km and } \lambda_{earth} = 3 \text{ cm}$$

There is not time to explain how the astronomical consequences are deduced. The results are: (1) The gravitaional red-shift affects γ (in the way that a clock runs slower in the presence of the gravitational field; it is the same under both hypotheses); (2) There is the well known motion of advance of perihelion (the same under both hypotheses; for Mercury this amounts to 43."0 per century); (3) Light is deflected by a gravitational field (1."75 at sun's limb). There are also results of smaller consequence: (4) The eccentricity of an elliptic orbit determined by angular motion is not the same as the eccentricity determined by distance measurement; (5) The value of the constant of precession determined by analysis of proper motion of stars is not the same as the value determined by measuring the earth's mass and moments of inertia by direct measurements on the earth itself. This effect was called by de Sitter "the geodetic precession" and is equal to 1."915/century in the case of a body revolving about the earth. This introduces a motion into the moon's perigee and node. The observations of this motion in the 1917 work are not precise enough to use as a general test of the theory, although when Eckert's work is finished it may allow a check. (The effect is the same under Hypotheses A and B.) (6) There is an additional motion of the perihelion--not due to the static gravitational effect of the field but to the rotation of the central body (also the same under Hypotheses A and B). This effect, in the case of the moon's orbit about the earth, is equal to 0".06/century. It becomes larger in the case of artificial satellites; however, it is not observable because of overcoming effects of the earth's atmosphere.

These are the complete consequences. Taking Hypothesis A versus Hypothesis B, there is only one difficulty: the precise value of r, the radius vector, plays an important part.

Under Hypothesis A the "coordinate" velocity of light is:

$v = c[1 + 1/2\ \gamma(1 + 2\cos^2 V)]$, where V is the angle between the light ray and direction to the sun.

Under Hypothesis B:

$v = c(1 + \gamma)$; the "coordinate" velocity of light is the same in all directions.

Herget then called for working sessions to draft resolutions on the following topics:

1. geo-unit
2. necessity for the updating system
3. optical tracking of deep space probes
4. pre-study of missions and input data

The following resolutions were drafted:

1. Recommend that a geocentric gravitational constant, termed k_e, will be fixed by fiat that is similar to the heliocentric Gaussian constant and will remain a constant by definition. This unit in turn will define a unit of distance for geocentric (and perhaps planetocentric) orbits which will be termed the "geo-unit." A numerical value will be recommended after recomputation according to Herrick, Baker, and Hilton's paper. Adopted values are to be held as approximate to but distinct from the physical shape and size of the earth.

2. Recognizing the distinction between the requirements for a consistent system of constants which may be used uniformly for computational purposes to facilitate direct comparison of results, and the requirements of individual users in specific cases, recommend formation of a continuing committee to provide, coordinate, and disseminate numerical values of astronomical and other physical constants, to describe their attributes, to advise concerning consistency, and to maintain records of values which are used for particular applications. It is recommended that the membership include a representation of the users.

3. Recommend that a formal study be made concerning the feasibility and usefulness of optical tracking of deep space probes and other space vehicles. This study to include both a theoretical investigation of the precision of orbits determined through simultaneous optical-angular and radar range and doppler data, and an experimental investigation of the feasibility and limitations of the optical tracking technique itself, with the standard of $0\overset{''}{.}1$ required.

4. Recommend that every group with a primary responsibility for space missions review the possibility of utilizing the mission to provide additional observational data for improving knowledge of astronomical and physical constants wherever possible.

After reconvening in plenary session, the participants agreed to adopt these resolutions as a consensus. With regard to resolution No. 1, Clemence abstained because of the need for clarifying its potential influence on lunar theory. These resolutions were to be conveyed informally to the scientific community as an expression of considered opinion of the symposium participants.

PLENARY SESSION
Monday Afternoon, 2-26-62
(A. Wilson, Chairman)

XII. RADIO AND RADAR DETERMINATION OF THE ASTRONOMICAL UNIT

Carpenter Discussion of the JPL radar experiment to determine rotation of Venus

The investigation consists of examining the returned 2388 Mc signal (continuous wave) from Venus to ascertain surface features. The technique is to compute the spectra (approximately 10 separate spectra for each 15 minutes of recording time). The relative-velocity doppler spread is tracked out by means of an ephemeris-controlled oscillator that is tied into an atomic clock. From the assumption of a cosine-squared law similar to the scattering of the moon at optical wave lengths, it is possible to deduce the scattering characteristics of the Venus surface. Our tentative result is that the rotation of Venus must be very slow and in a backward direction. Future projects include going up to 8448 Mc, increasing power to 25 kw and determining the scattering law of surface if possible.

Wong Discussion of STL dynamical determination of the a.u.

Our report* describes a dynamical determination of the a.u. based on the doppler data collected from Pioneer V between March 11 and June 26, 1960. The results depend on the data weights used in the least square determination. The data weights are presented on p. 34 of the report. Unfortunately our choice of the polynomial RMS data weight may not necessarily have been the best choice although the resultant value

$$\text{a.u.} = 1.49545 \pm 0.00015 \times 10^8 \text{ km}$$

tends to confirm the measurement of Rabe.

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*McGuire, J.B., and L. Wong, A Dynamical Determination of the Astronomical Unit by Least Squares Fit to the Orbit of Pioneer V, STL report, May 15, 1961.

Appendix A

WELCOMING ADDRESS

by

George H. Clement

It is my pleasure this morning to welcome you on behalf of The RAND Corporation to the Working Symposium on Solar System Constants.

Many times our visitors are perplexed by just what is The RAND Corporation--so perhaps a few words of explanation are in order.

The Corporation is an independent nonprofit organization engaged primarily in research on problems related to national security. A major portion of our effort is under contract with the United States Air Force, the Office of the Secretary of Defense, the United States Atomic Energy Commission, and the National Aeronautics and Space Administration. In addition, we conduct research under grants from the National Science Foundation and private foundations and with our own funds.

The organization was conceived out of the need at the end of World War II to develop a nucleus of the nation's scientists to work full time on problems of national defense, and the organization had its beginnings in 1946. Its affairs are governed by a Board of Trustees representing science, industry, and the public.

Our research program is concerned with the development of methods of scientific analysis and their application to the multi-faceted problems of long-range planning methods that consider many possible approaches and seek to determine those which may be preferred.

Research results are contained in reports and monographs, papers, and books. Distributions of some of these, because of the classified nature of their content, is limited to the government and government contractors. However, much of the research is unclassified, and reports on the results of this research are made available to the public through limited free distribution, through a system of library deposits in this country and abroad, through publication in the scientific literature, and through the bookstore sale of commercially published works.

Our interest in the solar system dates back to the very beginning of the organization, when in 1946 we published our first report "The Feasibility of a World-Circling Space Ship." Over the succeeding years we have gradually expanded our range of interest, and in 1956 published a paper entitled "Motion of a Small Body in Earth-Moon Space." This brought us face to face with the problems you will be addressing over the next five days. We hope that with your help we can continue to push the frontiers of our thinking further into the domain of the solar system.

It is my pleasure, at this time, to turn the meeting over to the Symposium Chairman, Al Wilson.

Appendix B

INTRODUCTORY ADDRESS

by

Albert G. Wilson

Let me, on behalf of the Planetary Sciences Department of RAND, add our welcome to George Clement's. In addition to George's summary of RAND's long interest in fundamental solar system constants, let me mention also that this conference itself is an outgrowth of the long association of Gerhard Schilling with this problem. He has perhaps had one of the best opportunities available to any scientist to see first-hand the unfolding of the demands of space flight on the fundamental constants of the solar system. His association with the work of the Smithsonian Astrophysical Observatory during the period when the techniques and procedures of satellite acquisition and tracking were first being synthesized and his subsequent role as chief of astronomy and astrophysics programs at NASA brought him into daily contact with the sort of astronomical information and scientific data which space flight planners needed from astronomers and geophysicists--information and data which did not always exist. Early in 1957, Dr. Schilling organized the first symposium to be held on fundamental constants for space flight uses, bringing together astronomers, geodesists, and space mission planners at the Smithsonian Observatory--several months before Sputnik I. One of Gerhard's first enterprises after coming to RAND was to plan a research project covering these areas where the new demands for astronomical and geophysical data had passed beyond the available supply--both in scope and accuracy.

The present symposium, organized under this project, is primarily for the purpose of taking a careful look at the astronautical requirements in the area of fundamental solar system constants and determining how to proceed in meeting these requirements. We are not meeting as an official body representing any official organization. We are not here to perform a judicial role, making any official decisions. We are here merely to explore together what we, at this time, feel the future role of ephemerides and fundamental astronomical and geophysical constants might be in the space age, and to explore possible approaches to these new roles. Although we want our discussions to be free to go in any fruitful direction, we are hoping to focus on the present situation with regard to the fundamental constants and explore what improvements are most important and what may be done to effect improvements.

I want to stress that this is a working rather than a reporting symposium. The reporting we do here is primarily to establish points of departure and to suggest directions of effort (reviewing past research primarily for this purpose). Since this conference takes place near the beginning of our investigations, it is best that it be as informal as possible. No record is being made of the discussions other than your own notes. Everyone is free to raise any question, explore any solution--it is all off the record. The ideas you bring and which you generate here will help guide us all. I am confident each of us will take away much more than he brought.

Appendix C

EDITORIAL NOTES

1. (p. 1) The term fundamental constants as used in astronomy applies to those constants belonging to a set in which there exist theoretical coupling relationships. These relationships consitute a model of the motion of celestial objects. It should be remembered that any list of fundamental constants is not unique. The classical treatments of this subject are Harkness (Ref. 9), Newcomb (Ref. 29), and de Sitter (Ref. 32). Summaries of values of constants are found in Allen (Ref. 1), Makemson, Baker, and Westrum (Ref. 25), and the Explanatory Supplement to the Ephemeris (Ref. 40). A general collection of papers discussing this subject in detail is found in the proceedings of the Paris Symposium on Fundamental Constants of Astronomy (Ref. 39). Details and discussion of the system of astronomical constants can be found in Chapter 3, "Orbits and Masses of Planets and Satellites," Brouwer and Clemence (Ref. 21).

2. (p. 2) By consistent set, the astronomer means that the theoretical relationships between the different constants are satisfied rigorously while the adopted value of each individual constant agrees with its observed value within the limits of the uncertainty of the observation (see discussion by Clemence on page 6).

3. (p. 3) The definitions of the a.u. and solar parallax are basic to much of the discussion following. "The astronomical unit of distance is derived from the adopted units of mass and time. If m is the ratio of the mass of any planet to the mass of the Sun, n the observed angular mean motion of the planet expressed in radians per day and k the Gaussian constant of gravitation, being 0.017 202 098 95 exactly, then a in the equation

$$n^2a^3 = k^2\ (1 + m)$$

is expressed in astronomical units. This equation may be regarded as the definition of the astronomical unit." (From "Orbits and Masses of Planets and Satellites," Brouwer and Clemence, Ref. 21) The solar parallax is defined as the angle subtended by the Earth's equatorial radius at a distance of 1 astronomical unit. For a discussion of the definition and astronomer's use of the term, solar parallax, see Herrick, Ref. 12, pp. 12-14.

4. (p. 5) This question refers to the precise determination of time made possible by the recent development of atomic oscillator clocks. The detailed determination of the variation in the speed of rotation of the Earth has been obtained by comparing time based on cesium standards which are stable to about 1 part in 10^{10} to Universal Time determinations (Markowitz, Ref. 26). Ephemeris Time (introduced by the International Astronomical Union in 1958 in order to provide a measure of time that is defined by the laws of dynamics) is based on the orbital motion of the Earth about the Sun rather than on the

rotation of the Earth. Beginning with the 1960 issue, the fundamental ephemerides of the Sun, Moon, and planets give tabular arguments of Ephemeris Time, the annual value of ΔT ($ET = UT + \Delta T$) are given on page vii or viii of the American Ephemeris and the numerical values of ET and UT differ only slightly. ($\Delta T = 34$ sec in 1962.) A complete discussion of definitions and the practical determination of time is given in the Explanatory Supplement to the Ephemeris (Ref. 40).

5. (p. 14) A more detailed discussion of relativity effects on astronomical observations is given by Clemence in a following session (Section X).

6. (p. 19) The subject to follow was discussed in greater detail in a Working Group Session (Section V).

7. (p. 25) Irene Fischer discusses the lunar distance determination by radar techniques in her paper "The Parallax of the Moon In Terms of a World Geodetic System," (Ref. 8). Preliminary results published by Yaplee are given in Ref. 37.

8. (p. 29) For final resolution drafted by this group, see Section XI, p. 61.

9. (p. 34) Since this time, Dr. Herrick has been appointed convener of the COSPAR Ad Hoc Committee on "Constants and Ephemerides."

10. (p. 38) This work is summarized in Ref. 13.

11. (p. 39) A discussion of the problem of lunar coordinates and its relevance to the determination of the physical libration of the Moon is given in Ref. 20 (see especially pp. 13-20).

12. (p. 41) All values of planetary diameters are given in the standard form of an angular measure (i.e., sec of arc) which is reduced to unit distance = 1 astronomical unit. Values cited here are from an analysis of all data which is being prepared for publication. For a complete list of published observations of the diameter of Mars, see Ref. 19, p. 29. For diameter of Mercury, see the Report of Commission 16, "Physical Observation of the Planets," Translations I.A.U., Vol. 8, 1952, pp. 208-209; 1958, p. 250.

13. (p. 42) Since this symposium, Mac Donald has published a paper on the internal constitution of the inner planets.

14. (p. 43) The JPL study to deduce the rotation rate for Venus is discussed by Carpenter in Section XII.

15. (p. 51) Since the symposium, Tony Gabler of RAND has prepared a working paper on this subject, entitled, "On the Problem of Estimating Solar System Constants;" in addition, E. M. Boughton at STL has continued with the work outlined by Wong on page 63. Although publication is still pending by both of these authors, the preliminary results

indicate useful application of statistical concepts to astronomical constants.

16. (p. 53) This effect (due to Duncombe's corrections) on the motion of Venus is described in the JPL Report (Ref. 28) on the a.u. radar determination, pp. 13-14.

17. (p. 54) See discussion by Clemence in Section VIII.

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Appendix E

SUBJECT INDEX

Appendix F

NAME INDEX

*present at Symposium

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*present at Symposium

*present at Symposium

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*present at Symposium